bility of the bridge when erected would equal that of the best existing structures of that class.

The paper referred to contains six points of objection, which are treated in a general way without attempting a scientific criticism. This is to be regretted considering the importance of the subject. I take each point in succession. With regard to

I. I cannot see an objection to the novelty of a system, if, as n this case, the conditions are unprecedented, and if the author of the paper himself is compelled to recommend a system of striking novelty.

II. What, may be asked, constitutes the enormity of magnitude of a structural part? Is it the excessive proportion of strain in it arising from its own weight to that arising from other weights and forces? If so, it will be found that this proportion may here be still very small, although it may not be ignored, as sometimes is done.

III. The experimental knowledge hitherto derived from structures with rising degrees of magnitude has not upset the theories used in the calculations of strength. It cannot be asserted that the top flange of a common rolled beam, being a strut, we assume twenty times as long as it is wide, would be under a test load in a safer position against buckling than the top flange of the Ohio girder bridge, which is 510 feet long and 20 feet wide, or the bottom flange of the Forth Bridge, which is 675 feet long and from 32 to 120 feet wide.

IV. We constantly rely on the strength of long struts; they exist in all girders, and many of them are of the same importance for the strength of the girders as the links for the strength of a chain. The theory of their strength, imperfect as it is, is applicable to all with a fair amount of truth, and there is no reason why it should not be applied equally to the struts in the Forth Bridge.

V. Assuming that the dangers from wind-pressure during the erection do not concern us here, it would be interesting to hear from the author which parts of the erected bridge would probably give way first, and whether this would take place by crushing, shearing, twisting, or pulling actions. The leverage offered to wind by the long brackets would come into question only when the pressure is different on the two sides of a pier. The difference would produce a twisting action, which would exist in the central pier, but which could be obviated in the two side piers. The resisting leverage of the central pier is 270 feet, or about two-thirds of the acting leverage. Approximately the same proportion obtains with regard to the stability against tilting under uniform wind-pressure, while in the case of the Tay Bridge the proportion was less than one-third.

VI. It is highly improbable that Mr. Baker should not have calculated his struts; in his book on the strength of beams, columns, and arches, he gives a very intelligible deduction of the theory of long struts, which, although elementary and not so elegant as that by the author, seems original. I have found deductions of that kind in most English text books, while in books of foreign origin generally the equation of the line of flexure is taken as the starting point. Its approximate form is—

$$-\frac{M}{EI} = \frac{1}{\rho} = \frac{d^2y}{dx^2}$$

M being the bending moment at any point, E the modulus of elasticity, I the moment of inertia of the section of the strut, and ρ the radius of curvature. The integration gives the limiting weight W acting endways upon a long strut, as already Navier stated it,

$$W = \frac{\pi^2}{a^2} EI$$

where $EI = C$ in the Paper. This formula is not applicable to short struts, since W might exceed the crushing strength of the material. The limiting weight W^1 for short columns is therefore calculated with $W^1 = fp$, where f is the sectional area and p the pressure on the sectional unit. Unfortunately there exists among theorists a difference of opinion as to the proper value of p; some put for it the crushing strength, and others the limit of elasticity, and now and then there are controversies going on about this matter. Meanwhile it is impossible to mark the limit between short and long struts which theoretically exists. Practically, however, the limit is indistinct, and Rankine, Gordon, and others, taking this into consideration, have put the two formulæ together into one empirical formula for W'', the limiting weight for struts of any given dimensions.

$$W'' = \frac{W^1}{1 + \frac{W^1}{W}}$$

This formula embellished with some empirical coefficients gives good results for struts of ordinary proportions, and as the struts in the Forth Bridge seem to have ordinary proportions, it is quite safe to use it for their calculation. M. AM ENDE

3, Westminster Chambers, Victoria Street, S.W., October 24

HAVING read with interest Sir G. Airy's article on this subject in the last number of NATURE, I am glad to see that it advocates a suspension-bridge in lieu of the proposed structure. It may perhaps interest your readers to give the particulars of the Great International Suspension Bridge over the Niagara River, which supports a carriage-way and a railway-track above.

The length of span between the towers is 800 feet. There are 4 cables, each composed of 3640 wires No. 9 = ·155″ diam., without weld or joint; the cables are 10″ in diameter. All the wires of each cable were separately brought into position, so that each one bears its full share of the tension. When a cable had been thus built up, it was tightly served with soft iron wire to bind the 3640 wires together, and to preserve them from rust.

Since this bridge was built, great improvements have been made in the manufacture of wire. Whereas the resistance to tensile stress at the moment of fracture of the best qualities of iron wire, such as that manufactured at Manchester for this bridge, does not much exceed 27 tons per square inch of section, *hardened and tempered steel wire* can now be made in large quantities and in long lengths with a minimum resistance at the moment of fracture of 90 tons per square inch.

Steel plates, rods, or bars cannot be made in quantity with a higher resistance than 34 tons, or less than half that of wire. Hardened and tempered steel wire similar to that used in pianos is thus clearly the most suitable material for suspension bridges, and has been recognised as such in America, where it is to be used in the construction of the New York and Brooklyn suspension bridge, the span of which is the same as the proposed Forth Bridge.

Our English railway engineers, however, have not yet recognised the great advantages wire possesses over any other form of material such as bars, chains, &c., for resisting tensile stress, and the further advantages that wire can be tested more easily and made of a more uniform quality.

Some ten years ago I called on Sir T. Bouch, the former Engineer to the Forth Bridge, to point out the advantages of a tempered steel wire suspension bridge over any other form of structure for the Forth Bridge. The idea was, however, never worked out on paper. WILLIAM H. JOHNSON

Manchester, October 23

On the Alterations in the Dimensions of the Magnetic Metals by the Act of Magnetisation

I HAVE read with interest Prof. Barrett's paper in NATURE, vol. xxvi. p. 585. Between his results as to the effect of magnetisation on the dimensions of bars of iron, of steel, and of nickel, and those of Sir William Thomson's experiments ("Electrodynamic Qualities of Metals," Part VII., *Phil. Trans.* R. S., Part I., 1879) on the effects of stress in the magnetisation of bars of the same metals, there exists a remarkable analogy, which, however, seems to break down in the case of cobalt. According to these experiments (which, I may mention, were carried out under Sir William Thomson's direction by my brother, Mr. Thomas Gray, and myself), the effect of the application of longitudinal pull to a bar of iron, while under the influence of inductive force tending to produce longitudinal magnetisation, is, for forces lower than a certain critical value, called from the Italian experimenter who first observed it, the Villari Critical Value, to increase, and of the removal of pull, to diminish, the inductive magnetisation. When the magnetising force exceeded the critical value, these effects changed sign, and tended to a constant value as the magnetising force was increased.

Again, the effect of transverse pull, produced by means of hydrostatic pressure in an iron tube, is, when applied, to *diminish* the longitudinal magnetisation, and when removed, to increase it. We see, then, from Joule's result, confirmed by

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Prof. Barrett's, that the effect of longitudinally magnetising a bar of iron, or of increasing its magnetisation, is to increase its dimensions longitudinally and to diminish them laterally, so that the volume remains constant; and on the other hand, from Sir William Thomson's investigations, that the effect of increase of longitudinal dimensions in an iron bar is to increase, and of increase of transverse dimensions to diminish its longitudinal magnetisation.

This analogy holds also with reference to steel and nickel. In the case of bars of these metals, we found their longitudinal magnetisation to be diminished by the application of longitudinal pull, and Prof. Barrett has found that bars of the same metals undergo a shortening when their longitudinal magnetisation is increased.

In the case of cobalt, however, the results do not agree. The results for cobalt, given in Sir W. Thomson's paper, are somewhat anomalous, but they refer only to the effect of stress on magnetism in a bar which had been previously magnetised and then placed while being experimented on, under the influence of the earth's vertical force The results were therefore complicated by the effects of the stress on the residual magnetism. So far as these results go they bear out to some extent those found by Prof. Barrett, but further experiments, the results of which have not yet been published, prove that the effects of stress are the same as for nickel. This is the case at least for all but low magnetising forces.

The behaviour of cobalt and nickel throughout a wide range of magnetising forces, and under the influence of both transverse and longitudinal stress, will, it is hoped, be fully investigated in a continuation of Sir William Thomson's experiments, begun some time ago, and temporarily interrupted by other, and for the time being, more pressing work, but now about to be resumed.

I may mention that my brother and myself pointed out in NATURE, vol. xviii. p. 329, the applicability of a modification of Edison's Tasimeter to the measurement of the changes of dimensions produced in a body by magnetisation. We still think that this is perhaps the most simple method, and we have found it very sensitive for qualitative results. In our trials of it we have experienced some difficulty in obtaining a carbon button which would return after having been subjected to stress to the same resistance as before. The experiments of Prof. Mendenhall, however, show that the kind of carbon used by Edison in his Tasimeter possesses this property in perfection; and we hope soon by the use of this carbon to obtain quantitative results.

ANDREW GRAY

The Physical Laboratory, the University, Glasgow,
October 19

Aurora

AN aurora was seen at Croydon at about 7 p.m. on Wednesday, the 18th inst. Three streamers of a whitish colour could be traced distinctly across the whole of the sky while the moon was still up.

A. E. EATON

The Victoria Hall Science Lectures

THE popular science lectures at the Victoria Hall have proved quite sufficiently successful, so far, to make the managers wish to continue them, provided that the kindness of competent lecturers makes it possible to do so. There have been audiences each night of ab ut 600—small compared with what the building will hold, but not amiss for a Friday night, in a neighbourhood where (except on Saturdays) people think twice before spending a penny. Those who have been present, agree in describing the audience as a peculiar one, for whom greater simplicity is needed than for the audiences of mechanics' institutes, and the frequenters of penny science lectures in general. They are quite ready to attend and to be interested, and do not think an hour too long, provided the ball is kept constantly moving, but as to this they are very exacting, and any breakdown of the apparatus, however temporary, places the success of the lecture in serious danger. There are stamps and whistles of impatience at any pause, such as must occur in adjusting experiments, but these cease the moment the lecture proper proceeds. This impatience perhaps makes the sustained interest of a lantern more suited to the audience than the more varied but intermittent experiments.

It is to be wished these lectures could be more widely known among the artisan class, who have not too many opportunities of hearing sound popular science.

ONE OF THE COMMITTEE

Royal Victoria Coffee Hall, Waterloo Road, S.E., October

THE TYPHOONS OF THE CHINESE SEAS[1]

THIS work by the learned director of the Zi-Ka-Wei Observatory, consisting of 171 pages quarto, and eight illustrative plates giving the tracks of the twenty typhoons of 1881, may be regarded as the outcome of the recent establishment of meteorological stations over the regions swept by the typhoon. The typhoons of 1880, amounting to fourteen, were described by Father Dechevrens in a previous paper. These two papers, from the greater fulness and accuracy of their details, form a contribution of considerable importance to the literature of cyclones.

An examination of the tracks of these thirty-four typhoons shows that they generally have their origin in the zone comprised between the parallels of 10° and 17°, some of them originating in the Archipelago of the Philippines, but the greater number to the eastward of these islands in the Pacific. The first part of their course is westerly and north-westerly; they then recurve about the latitude of Shanghai, and thence follow a north-easterly course over Japan. During the first half of their course the barometric gradients are steepest, and the destructive energy of typhoons is most fully developed; but after advancing on the continent, and, particularly after recurving to eastward, they rapidly increase in extent, form gradients less steep, and ultimately assume the ordinary form of the cyclones of North-Western Europe. In illustration of the steepness of the gradients sometimes formed, it is stated that on July 15 a gradient occurred of 2·760 inches per 100 miles, or one inch to 36 miles.

Typhoons do not occur during the prevalence of the north-east monsoon from November to May. In 1881 the typhoon season extended from May 22 to November 29. In Japan the true typhoon season is restricted to August and September, the storms there during the other months resembling rather the ordinary cyclones of temperate regions. The tracks of the typhoons during the months of moderate temperature, May, June, the latter half of September, October, and November, are the most southerly; they lie flattest on the parallels of latitude, and present a great concavity looking eastward; but those of the warmer months, July, August, and the beginning of September, exhibit, on the other hand, very open curves. This seasonal difference in the form of the tracks, taken in connection with the general form of the recurving tracks of the West Indian hurricanes, which are less open than those of the Chinese seas, suggests a possible connection between the forms of these curves and the different distributions of atmospheric pressure prevailing over the continents at the time.

Of the new facts brought forward in this report, the most important perhaps are those which show that the typhoon tracks have the feature of recurvation as distinctly as the hurricanes of the West Indies and the Indian Ocean. The degree of recurvation and the relative frequency with which it occurs in the tracks of the cyclones of the Chinese seas, the West Indies, the Indian Ocean near Madagascar, and the Bay of Bengal respectively, are important features in the history of these storms, which such reports will do much to elucidate. We shall look forward with the liveliest interest to Father Dechevrens' future reports, which from the lines of inquiry already indicated may be expected to throw considerable light on the influence of extensive regions of dry air and of moist air respectively, and of elevated

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[1] "The Typhoons of the Chinese Seas in the Year 1881." By Marc Dechevrens, S.J., Director of the Zi-Ka-Wei Observatory, China. (Shanghai: Kelly and Walsh, 1882.)

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